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KNOWING HOW AND VALIDITY

By ERNEST GELLNER

THE *knowing how—knowing that* dichotomy is used by Professor Ryle to obviate the fallacy that all intelligent performances entail that the agent contemplates, considers or in any other episodic sense knows some proposition. There is however an alternative mistake, which is suggested and not guarded against by Ryle's treatment. This is a mistake of treating *knowing how* as a type of validation, as a legitimate answer to "How do we know?" questions. I do not know whether in fact Ryle himself commits this mistake. The Presidential Address to the Aristotelian Society, 1945, gives the impression that he does; the relevant chapter in *The Concept of Mind* does not. What I shall say is consequently only fair with regard to the earlier work. My reason for saying it nevertheless is that *The Concept of Mind*, whilst not repeating the mistake does not give any explicit warning against it either, and the mistake made by Ryle in the first, 1945, formulation is likely to be repeated by some at any rate of those who accept his main point. There seems to be already at least one instance of this happening in print, this being Mr. Toulmin's article in "Defence of Synthetic Necessary Truth".[1]

The case which I shall elaborate is roughly that there is a tendency, requiring to be checked, to make *knowing how* do what "intuitions" used to do. The temptation is seductive because the job done by intuitions, that of unquestionably answering with an air of finality the question about the way of validating some standard, always has a certain appeal. It is no doubt tempting to do this with the help of *knowing how* rather than of intuition in view of the fact that there clearly is such a thing as *knowing how*, whereas there is no such thing as intuition. Before showing that to make the notion of *knowing how* perform the function of intuitions is a misuse of it, I shall first of all try to show the non-existence of intuitions.

The confident assertion that there are no intuitions, that no-one has ever had an intuitive apprehension of rightness, an implication, deity or of anything else looks like a piece of unjustifiable dogmatism. It is sometimes said that if anybody appeals to intuitive evidence, we cannot refute him directly but can only hope to do so indirectly by drawing out some

[1] *Mind*, 1949; cf. esp. the penultimate paragraph.

unpalatable implication of what he intuits.[1] But as people seldom intuit what is unpalatable to them, and as after all it is no great trouble to eliminate any undesirable indirect implication of their special insight by means of an additional *hilfs*-intuition liquidating the embarrassing logical relation, we should not get very far that way. But there is another way, for the denial of intuitions is not, despite appearances, an ordinary negative existential proposition. If it were, it could indeed never be reliably established. But there is no need to set off on an endless collection of evidence that Smith, Jones, Robinson . . . do not have intuitions, for the philosophically important concept of intuition can be shown to be self-contradictory. There are indeed senses of " intuition " in which it is not self-contradictory, but only in the self-contradictory sense can intuitions perform the illegitimate task assigned to them. Such plausibility as theories of intuition do have derives, I think, from a concealed fluctuation between the permissible but innocuous, and the self-contradictory but powerful senses of the word respectively.

Intuition is required to be a something which is simultaneously *both* the evidence, in the sense of initial conditions of an inference *and* the guarantee of the legitimacy of the inference from the evidence. There tends to be actually a triple impossible identification—of evidence, the rule of inference from that evidence, and the guarantee of the truth of that rule. This identification is the essence of " intuition " and is shared by otherwise quite diverse myths. Consider, for instance, a non-Humean theory of causation, which will have to claim that the causal property is intuited in the cause or the effect in isolation. The logician will rightly point out to anyone holding such a theory that a causal inference cannot be made from the cause (the initial conditions of the inference) alone but only from it in conjunction with a generalisation. If the logician goes a step further and interprets the requisite awareness of the generalisation as necessarily episodic, he will be committing the mistake which Ryle is concerned to eradicate. But this obviously does not mean that the intuitional theory of causation was right, or that the logician was wrong provided he restricted himself to pointing out that causal inferences are not valid in virtue of the initial conditions only.

What holds of causation holds of ethical, aesthetic, theological, logical, and any other candidates for intuition equally. The intuitionist in morals, for instance, does not, I think, wish

[1] Cf. for instance Nowell-Smith: *Freewill and Moral Responsibility*; Mind, 1948, p. 53.

to say that being intuited as obligatory *is* what makes an obligation valid, if only because that would free anyone unburdened with such intuitions from obligation. Similarly, the mystic does not wish to blaspheme by saying that his special experience *is* God. What both would rather like to say is that there is a special experience, with an inference from that experience to *something more* than it; and that somehow the experience is a guarantee of the validity of that inference. But this only requires to be stated clearly to be seen to be impossible. The defender of intuitions may attempt to evade the argument by saying that it would hold if we were concerned with ordinary, natural experiences; but that here we have to deal with special non-natural ones. But the argument does not depend on the inclusion of intuitions in the category of natural events, however that category may be defined. It depends only on intuitions being something which happen to specific people at specific times.

What then makes theories of intuition plausible, and what are the legitimate senses of the word? To begin with, ordinary people, scientists, mathematicians, writers all obviously do have hunches; they do make inferences, state theorems, predict effects without thinking of the underlying rule, possibly without knowing it, and even more often without having proved it, in whatever sense if at all it is susceptible of proof. It is incidentally the fact that Ryle's central thesis is right and that rules are not frequently present as occurrences, which leads to the misinterpretation of the above phenomena as intuitions. If you wish to call them intuitions there is perhaps no great harm in it, provided it is clear that all you are doing in describing them as such is telling how a certain inference, guess or trick was or could have been carried out or arrived at—*i.e.* without extraction of the rule of procedure underlying it, without explicit formulation and proof of it. You are *not* giving a logician's account of why the inference or guess was valid or true respectively. The intuition in the harmless sense is valid in virtue of something or other, but *not*, like the mythical intuition, simply in virtue of having occurred.

There are also more specific kinds of phenomena making for the theory of intuition. There are all the irregular unique or unprecedented hunches, some accompanied by an irresistible feeling of conviction, which *turn out* to be correct. There are also those philosophically interesting topics where no-one has succeeded in eliciting the rules—we do not know the sufficient and necessary conditions of something being right or beautiful —and where there is reason to believe that these rules never

will be discovered. In the general cases of the preceding paragraph the validating rule *was not* present in an episodic sense; in all these cases for one reason or another it looks as if it *could not* be. But again, intuition in these cases is not valid because it occurs but in virtue of something else. A qualification is necessary here: if certain kinds of subjectivist theories are true, if the aesthetic or ethical hunch is not a clue to something else but simply *is* the aesthetic or ethical property, then it does become self-authenticating, by saying nothing. It again fails to be a *hunch* in a strong sense.

There is another special class of things which some people may hanker to grade as intuitions—gestalt-perceptions. When I see a pattern in a cloud or on a Rorschach card I do not see only what I saw there before I saw the pattern, but nor have I made an inference from what I saw there before; what happens is that I . . . well, words fail me; what happens is just that I see a gestalt. The reason why words fail me is not, I think, any deficiency of language or merely my insufficient command of it, but the survival of the prejudice that when we see something we must either see what we could have seen in the same object at any time or we must be inferring from it. This makes us attempt to give an account of gestalten in terms of one of these two, whilst the whole point of the discovery of gestalten is that the above dichotomy is not exhaustive. Gestalten are obviously important for ethics and aesthetics. Take the case of ethics; it is plausible to assume that judging a person, unlike perhaps making decisions about returning books by post, involves a kind of biographically synoptic perceiving of a certain pattern. Maybe some people or classes judge in a completely atomic manner, by adding up observances and breaches of isolated obligations. Some others perhaps judge in a fully teleologically unified manner, in terms of conduciveness to some solitary overriding aim. In fact I do not think either of these extremes often obtains. One usually judges in a manner which is a mixture of these types, supplemented by attention to a kind of emergent pattern sometimes called way or style of life. The perception of this element can be described as intuitive, but again, it is *not* such in the sense of being the validating factor. The fact that a certain pattern is noticed or perceptible is not what makes that pattern the desirable one. Misdemeanours and disapproved behaviour-patterns also have gestalten, *e.g. la vie bohemè*.

My motive for flogging the dying horse of intuitionism is not sadism alone. It is also that the tough-minded terminology

of *knowing how* is liable to be used for the same purpose in place of the comparatively unfashionable terminology of intuitions. What has happened is, I think, this: the misinterpretation of *knowing how* as *knowing that* was perpetrated initially by logicians and moralists who were primarily interested in questions of validity and only incidentally interested in the philosophy of mind. Still, their mistake in the latter field was liable to lead them into metaphysics which in turn were liable to affect their conclusions with regard to their main question. Ryle, on the other hand, interested primarily in the philosophy of mind, firmly eliminated the myth of the co-extensiveness of *knowing that* with intelligence, but in turn misinterpreted, at any rate in 1945, *knowing how* as a logician's category (a type of validation), or as a reason for why logicians' categories were inapplicable, which really amounts to the same, as I shall attempt to show. In reality the *knowing how—knowing that* dichotomy, all-important for the philosophy of mind, is not coordinate with, but on the contrary cuts across, logical classifications such as analytic, synthetic, synthetic a priori, pragmatically justified or unverifiable. Ryle in 1945 seemed to conclude that because something was shown to be a matter of *knowing how*, the question concerning which of the logician's pigeon-holes it was to go into was silly. But this is not so; consider, for instance, the case of inference. Ryle rightly points out that the notion of *knowing how* is required for explaining the source of Lewis Carroll's paradox in the story of Achilles' conversation with the tortoise. But the fact that a skill is required for the application of a rule of inference does not mean that the rule need also be a valid rule: the fact that it is applied, the fact that someone knows how to use it, does not by any means make it valid. Rules are not relevant to the novice's performances only. False or self-contradictory rules of deductive inference may be constructed and people may acquire the skill of operating them and manufacturing fallacies and contradictions. Any philosopher, for instance, who has spent some time discussing the logical antinomies has acquired such a skill.

Let us turn to the motives one might have for treating, in general or over a limited field, *knowing how* as a logical category, as a type of validation. I can think of three of these reasons, two of them incidental and possibly not operative, and the third the main and effective cause of the mistake. I shall discuss the two minor ones first.

Firstly, there is the bogey of scepticism. It is notoriously plausible to say with regard to quite a number of important skills, such as those of making inductive generalisations or

behaving correctly, that if we succeed in eliciting the appropriate rule, and translate the *knowing how* into explicit *knowing that*, we may find ourselves in a position of having to consign it, for purposes of logical classification, to the class of unproved or unprovable items of *knowing that*. Philosophers who dislike sceptical conclusions do so for at least two reasons: firstly, the traditional one that they sometimes feel that their function is the justifying of beliefs, and secondly for reasons connected with some aspects of contemporary philosophical method. The effect of Professor G. E. Moore's habit of asking "*what* are x?" instead of "*are* there x?", reinforced by the theory that philosophy is concerned with language as used, has been the tendency to rule out sceptical conclusions *a priori*. It is of course possible to philosophise in such a manner that one cannot arrive at sceptical conclusions, just as it is possible to produce the illusion of increased income by printing money. But when sceptical conclusions are ruled out beforehand by the very method employed in discussing the problem, the validation of the cherished beliefs becomes of a rather dubious kind, like an increased income produced by a doubling of salary accompanied by a doubling of prices. Some people, as is known, are nevertheless happy in such situations. But the use of *knowing how* to answer scepticism does not constitute a merit. Scepticism in various fields seems to me a theory and not necessarily the apodosis of a *reductio ad absurdum* argument. It may well be an important feature of some human activities that the rules underlying them cannot be justified.

It is worth mentioning at this point that one of the functions of language as the most important, or possibly the necessary, conditions of *knowing that* is its job of facilitating criticism, comparison, discussion and improvement of skills. Unverbalised skills are by their very nature unobservable, except when actually employed, and then exercise of them is liable to inhibit observation; they can be developed and improved only by the laborious method of trial and error. Those, however, which by means of verbalisation can be pinned up in a glass case can often be compared, modified or rejected far more speedily and efficiently. I am not, incidentally, denying that some untranslated *knowing how* must always be present, even in the verbal discussions, this being true for all the reasons connected with Lewis Carroll's paradox.

The second reason is that with regard to certain topics, such as art or sport, the extraction of the rule, the translation of *knowing how* into *knowing that*, may be very difficult, or im-

possible, and whether it is successful or not, the attempt itself may be unhelpful or even harmful to the performance. When dancing or playing table-tennis, it is fatal to think about what one is doing. We need only remember that unfortunate case of the centipede. Another but similar trouble may arise in ethics. I gather that certain social groups subscribe to ethical codes which incorporate as one of their rules the prohibition or discouragement of ethical introspection and critical speculation; to ruminate about whether what *isn't done* really is not to be done may itself be a thing that isn't done. All these cases, where *knowing how* occurs, but *knowing that* is difficult, undesirable or undesired, are liable to tempt one into misconstruing *knowing how* as a category of validation, or into making its presence a reason why no validation is necessary, which, as I have suggested, amounts to much the same. For some genuine logical category of validation, of *reason for selection*, in effect, must always apply *wherever there is a choice from amongst alternatives*[1] if only that residual category signified by the pejorative term "arbitrary". There *are* alternatives in painting a picture or dealing with a situation, and therefore . . . Q.E.D.

The final and main reason for misusing *knowing how* as a category of validation is the ambiguity of 'knowing how' and similar expressions. To say that somebody knows how to do something is to say two things: that he can *do* it, and that he can do it correctly. This can best be seen from considering a denial, *e.g.* "he does *not* know how to argue". This may mean either that like the tortoise in Lewis Carroll's story, he simply isn't capable of making the step and inferring from the premises at his disposal; but it may equally well mean that he does make the step, but the wrong one and comes to a conclusion which does not follow. When we assert that he *does* know how to argue we are simultaneously denying both these possibilities. One may fail to notice that two things are being said because 'knowing how' is never predicated unless both hold, and the second can't hold unless the first does.

To say that two things are meant by 'knowing how' is not the same as to say that two processes are referred to by it, this being the fallacy which Ryle rightly attacks. Only *one* process is indicated, but what is *also* being said is that the process satisfies some criteria; this is mentioned by Ryle in *The Concept of Mind* but ignored, and I think implicitly contradicted, in the 1945 paper. To say that someone knows how to

[1] This is analytic—for a "type of validation" is just the kind of way we choose the true or valid proposition from amongst possible alternatives.

argue is, apart from mentioning his disposition to behave in a certain way, also a statement that what he does when the disposition is actualised is an instance not merely of argument, but of *valid* argument. It is to say that he will observe the appropriate rules; "observe" not indeed in the sense of "look at" but in the sense of "act in accordance with". The denial of the "two processes" theory should not blind us to the duality of meaning of expressions such as "knows how to . . ."

This point can also be made by saying that *knowing how*, like, for that matter, "knowing" *tout court*, is an expression for achievements. "Knowing how" is, to be precise, a triply laden expression: for when I say "Tommy knows how to skate" I (i) refer to a disposition of Tommy's to do what is describable as skating (ii) imply that he succeeds, which (iii) prejudges to some extent the question of what the criteria of good skating are, by entailing that they are such as are satisfied by Tommy's performance. Only (i) refers to a process, indeed; but important as it is to notice this, it is also important not to ignore (ii) and (iii). The sense in which "knowing how" is presupposed in all knowledge, which Ryle insists that it is in the 1945 paper, is sense (i) only. There are at least two reasons why there is a tendency to confuse the various senses: firstly it appears that we tend to be interested only in successful performances and have no terms corresponding to "knows how to . . ." but possessing only its minimal force. The nearest (axiologically) neutral term seems to be just the humble word "doing". The second reason is that *some* words for actions, but some only, analytically entail what the criterion of successful performance is. 'Playing football,' for instance, does, though 'skating', 'composing' or 'behaving oneself' do not; the activity of playing football *is defined* partly in terms of trying to score goals or tries, *i.e.* in terms of the criteria of success. In the case of such activities the distinction that I have drawn are not relevant. I shall, whenever the distinction becomes important, refer to the neutral sense of 'knowing how' which begs no questions of validity as sense (1), and to the strong sense as (2).

Toulmin, in the article mentioned above, does seem to think that *knowing how* has a special relation to synthetic necessary truths. The suggestion would seem to be that the latter are either valid in virtue of being kinds of *knowing how*, or that for this reason the question of validity does not arise. If this were true it could undermine my case that Ryle's dichotomy is irrelevant to classifications of logical form. To begin with, Toulmin

classifies as synthetic a priori all those propositions which are necessary but with regard to which the guaranteeing definitions are not easily available but would have to be specially constructed. Well, Toulmin can if he wishes use ' synthetic necessary ' in this way ; indeed he explicitly abstains from claiming that the kind of proposition he is interested in corresponds exactly to the type normally discussed under that title, by christening them ' Type-Q propositions ' ; but it is worth noting that in *that* sense language will be overcrowded with synthetic necessary truths,[1] the vast majority of which will not be of the kind which is normally discussed in connection with the problem of synthetic a priori propositions. To say that language is overcrowded with Toulmin's type of synthetic necessity is to say that language is full of entailments which no-one has bothered to formalise and which it would be unnatural to formalise. Really plausible candidates for synthetic necessity are not those entailments with regard to which the definitions have not been in fact constructed, but those where the construction of the definitions comes up against certain problems. If a term A figures in an entailment "A entails B" and we *can* construct a definition of A which including " entails B " and a definition of A′ which resembles the definition of A in all respects *except in so far as it does not include " entails B "* ; and if it is clear that A′ *does not entail B*, no problem arises and most of us, though not Toulmin, are satisfied that the entailment is analytic. This procedure, however, does not always work, at any rate easily, and it is when it doesn't that there is a point in talking of a genuine candidate for synthetic necessity. For instance, I can define " red " as " like the Soviet flag, and not green " (definition A), or alternatively as just " like the Soviet flag " (definition A′). Unfortunately the entailment " not green " still holds even for A′, and therefore cannot be attributed to its inclusion in the definition A. This kind of difficulty has, for instance, been very clearly exhibited by Mr. D. F. Pears[2] with regard to certain temporal features.

Toulmin says that his synthetic necessaries have something to do with *knowing how*. Yes, indeed, for he has defined his class of them in terms of lack of formalisation ; and to say that something is a matter of " knowing how " *is* to say that it is an intellectual but unformalised, unverbalised operation or that the verbalisation is irrelevant. *Knowing how* has come in not

[1] Toulmin must, however, obviously also imply that there is some kind of correspondence between ' Type-Q ' and synthetic *a priori* by discussing it in connection with the forms, the general polemic concerning the latter.

[2] *Time, Truth and Inference* Proc. Arist. Soc., 1950.

because of synthetic necessity but because of the lack of formalisation. But the weak *knowing how* (1) is present equally in unformalised statements of any kind, synthetic, analytic and the rest. For they all involve doing, if only operating with undefined symbols or recognising applicability or suitability without explicit criteria. More important still, this *knowing how* (1) is also present in false statements. *Knowing how* (2) is *not* necessarily involved, unless Toulmin restricts himself to *successful* or *correct* exercises of the kind of *know-how* which he considers to be of 'Type-Q', and then of course it will be present by definition, for *knowing how* (2) = *knowing how* (1) plus success, by some criteria. It is the smuggling in of these criteria in not unpacked *knowing how* (2) that is the danger. Hence in neither sense is there a special connection between *knowing how* and synthetic necessity such as Toulmin suggests.

Toulmin seems to believe that the opposition to the doctrine of the synthetic a priori is based primarily on dislike of the "inner eye", special insight, etc. This belief of his partly accounts, I think, for his keenness to introduce *knowing how*, for he seems to think that if *knowing how* replaces special intuition the basic objection will vanish. But even if special intuition were always relevant, which it is *not* in Kant, who after all matters most in the context of synthetic a priori propositions, *knowing how* would not be an eligible heir. To say that a proposition is synthetic or analytic or of any other kind is to say what kind of proof would be seen to be relevant to it *if* the appropriate definitions *were* constructed; "intuited" was a spurious addition to this list, sometimes coupled with synthetic a priori. It is *not* to say anything whatever about whether the "rule" of the operation *has* been made explicit.[1]

In his final paragraph Toulmin points out, talking of mathematicians' and logicians' shortcuts, that intuition is a good thing but no substitute for proof. True enough; but one should stress the same about *knowing how*, which also is not the *kind* of thing that can deputise for a proof. I am not claiming that all skills made explicit *are* susceptible of proof; and admittedly those which are so may be so in different senses. Nor am I denying that what have been claimed to be synthetic a priori principles, *e.g.* causation, may perhaps be illuminatingly described as performance rules rather than as propositions. What I want to guard against is the possibility of *knowing how* being in any way used to silence or "solve" discussions of validity or to

[1] Cf. for instance, G. Frege, *Foundations of Arithmetic* tr. Austin, p. 3. B. Blackwell Ltd. Oxford, 1950.

suggest that these are based on a misapprehension. *Knowing how* is an excellent thing, but it is no substitute for proof, evidence or grounds, nor, alternatively, for admission of arbitrariness.

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A NOTE ON SUBJUNCTIVE AND COUNTERFACTUAL CONDITIONALS

By ALAN ROSS ANDERSON

SEVERAL recent articles[1] have stated or implied that true subjunctive conditional sentences entail the denial of their antecedents. Hampshire, for example, says " . . . the subjunctive form *explicitly implies* that the condition specified is contrary to fact . . ."[2] Chisholm has mentioned that this fairly widespread opinion leads to some difficulties, but does not dismiss it completely.[3] It will be the purpose of this note to establish conclusively that this view is untenable.

It is generally felt, by those who construct logical analyses of every-day language, that tense indications in English verbs are elliptical expressions—or at least that they may, for certain systematic purposes, be treated as such. We give this type of explanation when confronted with the apparent fact that the sentence " It is raining in Cambridge " is sometimes true and sometimes false. Actually, we say, the sentence in question should be regarded as elliptical for " It is raining in Cambridge at time t ", where t is ordinarily the time of utterance. Similarly, the sentence " It was raining in Cambridge " is regarded as meaning " It is raining in Cambridge at time t ", where t is some time previous to the time of utterance. Tensed verbs, in other words, may be replaced by tenseless verbs and an explicit time indication. On this view the non-temporal character of truth is retained, and a sentence such as " It is raining in Cambridge on January 1st, 2000 A.D. " is regarded as either true or false—we just don't yet know which.

In compound expressions involving logical connectives, such as the material conditional, the same technique is used. We might say, for example, that the sentence " If it was raining

[1] See Pears, " Hypotheticals ", *Analysis*, X (1950), 49–63; Hampshire, " Subjunctive Conditionals ", *Analysis*, IX (1948), 9–14; Weinberg, " Contrary-to-fact Conditionals ", *Journal of Philosophy*. XLVIII (1951), 17–22.

[2] *loc. cit.*, p. 11.

[3] Chisholm, Review of Pears, *op. cit.*, *Journal of Symbolic Logic*, XV (1950), 215–216.

in Cambridge yesterday, then there were clouds in the sky" means "If it rains in Cambridge on May 8, then it is cloudy in Cambridge on May 8". (We stipulate that the material conditional is intended in the sentence quoted).

A complete analysis of the tenses of English verbs along these lines is rather complicated,[1] but this rather brief description of the approach is sufficient to discuss a difficulty which arises when the method is applied to subjunctive conditionals. The following two sentences are illustrative:

A: If Jones had taken arsenic last year, he would have died.

B: If Jones were to take arsenic next year, he would die.

A grammarian would say that *A* and *B* differ only in tense, and according to the proposal mentioned above, both would be regarded as elliptical for

C: If Jones takes arsenic at time *t*, he dies at time *t*, *t* is any time during 1950 in case A, and any time during 1952 in case *B*. (We will assume that arsenic acts instantly). Then *A* and *B* are regarded as meaning

A′: If Jones takes arsenic in 1950, Jones dies in 1950.

B′: If Jones takes arsenic in 1952, Jones dies in 1952.

A problem arises from the assumption that there is a difference in the logical characters of A and B. We may feel entitled to infer from A that Jones did not take arsenic last year (*i.e.*, we may regard subjunctive conditionals in the past tense as counterfactual), but we would not feel inclined to infer from B that Jones will not take arsenic next year. It is clearly wrong to say that we can infer the denial of the antecedent from a subjunctive conditional in the future tense,[2] but this rule of inference seems more plausible when applied to subjunctive conditionals in the past tense.

Suppose we still take the view that we *can* infer from A′ that Jones did not take arsenic in 1950, while admitting that a similar inference is not valid for B′. Then suppose that Jones in fact does take arsenic in 1952, and dies. Then in 1953, we should be able to infer from B′ (since it would then be in the past) the false conclusion that Jones did not take arsenic in 1952. But then it would be the case that B′ was true up until 1952, and false thereafter, and the non-temporal character of truth (which this way of dealing with tenses was designed to retain) would again be lost.

Now it might appear possible to save the situation by rejecting A′ and B′ as analyses of A and B. We might hold that the

[1] See, for example, Reichenbach, *Elements of Symbolic Logic*, § 51. New York, 1947.

[2] This is the point that Chisholm makes; *loc. cit.*.

tense of verbs in subjunctive conditionals makes a stronger contribution to the sense of the sentence than does the tense of verbs in indicative assertions. According to this interpretation the sentences :

D : If Jones *were to take* arsenic in 1952, he *would die* in 1952.

and

E : If Jones *had taken* arsenic in 1952, he *would have died* in 1952.

would have different analyses. From E we should be able to infer that Jones did not take arsenic in 1952, but we should not be able to make that inference from D.

But even if the difficulty could be avoided by abandoning the view that future and past subjunctive conditionals are of similar logical structure, it seems apparent that *any* analysis which allows the inference from the truth of a subjunctive conditional in the past tense to the falsity of its antecedent would not adequately describe our intended meaning. The following example will be sufficient to indicate a large range of cases in which we would not make such an inference. In the investigation of Jones' death, a doctor might say, " If Jones had taken arsenic, he would have shown just exactly those symptoms which he does in fact show ". Now in this context the doctor's statement would probably be taken as lending support to the view that Jones took arsenic—it would certainly not be held to imply that Jones did not take arsenic.[1]

Such examples indicate that it is incorrect to say that we can infer the falsity of the antecedent from a true subjunctive conditional in the past (or any) tense.

Now suppose that we wish to consider the properties of a special kind of conditional, the counterfactual conditional " c ", which differs from the indicative and subjunctive conditional in that it entails the denial of its antecedent, *i.e.*, which is such that " AcB " entails " $\sim$A ". We can get this effect immediately by defining the counterfactual conditional in terms of the subjunctive conditional " s " ; *i.e.* " AcB " is defined as " AsB & $\sim$A". The uses of conjunction and denial are no more obscure in this context than in any other, so we may conclude that a counterfactual conditional presents no difficulties of logical analysis beyond those of the corresponding subjunctive conditional. The problem of giving an adequate logical analysis of

[1] One of the pragmatic features of the subjunctive conditional which makes Hampshire's view more plausible is that we most frequently use subjunctive conditionals in the past tense in contexts where we are prepared to deny the consequent. In these cases, we are simply applying implicitly the rule of *modus tollens*, which holds for the subjunctive conditional as well as every other relation of conditionality.

counterfactual conditionals is thus " reduced " to the problem of giving an adequate logical analysis of subjunctive conditionals.

According to this view, then, (i) subjunctive conditionals do *not* entail the denial of their antecedents. (ii) To every subjunctive conditional sentence there corresponds a unique counterfactual sentence, and conversely; and further (iii) the class of counterfactual conditionals and the class of subjunctive conditionals are mutually exclusive. Finally, (iv) an adequate logical analysis of subjunctive conditionals (or of some subclass of the subjunctive conditionals) will yield as a trivial consequence an adequate analysis of counterfactual conditionals (or some subclass of the counterfactual conditionals.)

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MR. BLACK ON TEMPORAL PARADOXES

By RICHARD TAYLOR

MAX BLACK in his recent paper[1] has shown that the ancient puzzles of time and motion, associated with Zeno, may still be with us, and that novel proposals for solving them are still possible. The present discussion is prompted, however, by a belief that Mr. Black's own proposed solution is dogmatic, that it generates as much paradox as it is intended to remove, and that, in fact, the original difficulty *can* be overcome by pursuing the suggestion adumbrated by Aristotle.[2]

Mr. Black maintains (1) that the paradox does not result from a mathematical fallacy, (2) that the notion of " an infinite series of acts " is an impossible one, and (3) that the difficulty really arises from confusing a process itself with some mathematically " useful " way of describing it, (p. 101); once this confusion is exhibited, the paradox is supposed to vanish.

I shall say little with respect to the first contention, other than that Mr. Black seems to be at least partly correct. Through the summation of a geometrical progression one can specify at what point a process of the sort described by Zeno will terminate, but the question that remains, and the only one Zeno asked, was how such a *process* can be consistently conceived in the first place.

[1] Achilles and the Tortoise, *Analysis*, Vol. II, pp. 92–101.

[2] Aristotle proposed to remove the paradox by distinguishing between infinitude of extent and that of divisibility, (*Physics*), 233a. The passage in question is quoted from by Mr. Black, without comment, in a footnote, p. 91.

The second claim appears to me not only unproved by Mr. Black, but clearly mistaken, inasmuch as the "absurd consequences" he draws from his excellent illustrations, by which means alone he supports his contention, all tacitly rest upon a self-inconsistent notion of "an infinite collection", viz., that of a collection which, in a sense I shall try to illustrate, both has and has not a last member.

We are invited to consider a hypothetical machine which transfers marbles from one tray to another, requiring one minute to move the first, then a minute to rest, a half-minute for the second, then a half-minute to rest, a quarter-minute for the third, with a quarter-minute pause, and so on. Now the question arises, though it is not raised by Mr. Black, how many marbles the machine will finally have "counted" at the end of four minutes, assuming its supply to be sufficiently copious not to be exhausted before then. The answer clearly *seems* to be that it will have counted infinitely many of them—viz., one marble for each member of the series 1, ½, ¼ . . . etc. At any rate, no *less* than an infinitely large supply would suffice; otherwise, the marbles would certainly be gone before four minutes were up.

Mr. Black neither asserts nor denies this, so far as this machine is concerned; what he does remark is that at the end of four minutes the machine stops, "and we now see that the left-hand tray that was full seems to be empty, while the right-hand tray that was empty seems full of marbles" (p. 97). But this, I submit, is quite irrelevant, and betrays the impossible conception he tacitly entertains throughout. If, as supposed, the supply of marbles is infinitely large, and there is a first one, there cannot be a last one. If the machine simply removed every *other* marble—e.g., if those in the first tray were alternately red and black, and it took only red ones—there would be no danger of the available marbles becoming exhausted; yet there would remain uncounted all the black ones.

A second machine described functions like the first, but with only one marble; while it rests, after each transposition, another machine restores the marble to its original place on the left. Each uses a minute for its first transposition, a half-minute for the second, a quarter-minute for the third, and so on. This illustration, Mr. Black believes, "shows clearly that the infinite count really is impossible", for "the very act of transferring the marble from left to right immediately causes it to be returned again; the operation is self-defeating and it is logically impossible for its end to be achieved" (pp. 97, 98).

But *what* " end " ? If, as Mr. Black now seems to suppose, the " end " would consist of " counting " this marble over and over until no further countings *could* be made, i.e., until the possible transpositions from left to right were exhausted, then obviously *that* end cannot be achieved ; an infinite collection of objects, by definition, does not have, as Mr. Black seems to think it should have, both a first and a last (or " infiniteth ") member. But none of this is in the least inconsistent with the supposition that the machine will have moved the marble infinitely many times. To count out an infinite number of marbles from a sufficiently copious supply, a machine would not need to count the *entire* supply—half of them, for instance, would do.

If, on the other hand, the " end " referred to, and declared to be *logically* incapable of being attained, is simply to move the marble from left to right infinitely many times—and this, I took it, was the original supposition—then not only is this logically possible, it is *logically* necessary. If, in other words, the machine neither breaks down nor decreases its rate of acceleration—as by hypothesis it does not—then it cannot but move the marble infinitely many times ; viz., once, for each member of the series 1, $\frac{1}{2}$, $\frac{1}{4}$ . . . etc. That the marble should then be returned to the left, such that further transpositions would be possible (provided the machine were started all over again ,) is quite irrelevant to this.

Further in connection with this illustration Mr. Black remarks that " if the result of the whole four minutes' operation by the first machine is to transfer the marble from left to right, the result of the whole four minutes' operation by the second machine must be to transfer the marble from right to left. But there is only one marble, and it must end somewhere ! Hence neither machine can accomplish its task, and out description of the infinity machines involves a contradiction " (p. 98). But of course the second machine does not, on Mr. Black's hypothesis operate four minutes ; starting a minute later than the first when the first commences its pause, it runs only three. Apart from this, however, we may ask again what the "tasks" of these machines are conceived to be. Mr. Black again assumes apparently, that an infinite number of moves can have been made only when the last (or " infiniteth ") of an infinite number of possible moves has been accomplished, betraying the tacit conception of an infinitude having not only a first but also a theoretically discernible last member—i.e., an infinitude which is gratuitously thought to be not infinite after all.

Mr. Black subsequently modifies his illustration so as to have the machines operate with *two* marbles, passing them back and forth, and thus eliminating the pauses, taking a minute for the first exchange, a half-minute for the second, and so on as before. Here he notes that " if the first is to succeed, both marbles must end on the right, but if the second is to succeed, both must end on the left. Hence neither can succeed, and there is a contradiction in our description of the machines " (p. 100). But is this not wholly arbitrary? The supposition still seems to be that there *ought* to be a theoretically discernible last transposition of the marbles, whereas if, as was part of the hypothesis, the motions of either machine are to correspond with time intervals of the ratios 1, $\frac{1}{2}$, $\frac{1}{4}$ . . . etc., there assuredly can *not* be any such " last " movement. At any stage of the process, further movements are discernible.[1]

I suggested at the outset that Mr. Black's own proposal is dogmatic, and may engender more paradox than it removes. Let us now examine it, to see whether this is not so.

Having satisfied himself, by means of the illustrations just cited, that the notion of " an infinite series of acts " is " self-contradictory ", Mr. Black insists that a runner, after all, in moving from one place to another, is required to perform but a finite number of acts, i.e., steps, and that the path he traverses is composed of but a finite number of stones, blades of grass, grains of sand, and atoms (p. 101). Now Zeno can of course be assumed to have known this—he would not pretend that we would, upon trying to count them, find these steps, stones, etc., infinitely many. So far there is no dispute. Zeno would remark further, however, that each such thing is divisible without end; however far a stone, for instance, might be divided, so long as any part remains, that part too can be thought of as sundered.

Mr. Black anticipates this, and insists that it is really only an illusion arising from " confusing a series of acts with a series of numbers generated by some mathematical law " (p. 95). " If anybody says we must imagine that the atoms themselves occupy space and so are divisible 'in thought'" he writes, then " he is no longer talking about spatio-temporal things. To divide a thing 'in thought' is merely to halve the numerical interval which we have assigned to it. *Or else* it is to suppose what is in fact physically impossible beyond a certain point, the actual separation of the physical thing into discrete parts " (p. 101, italics supplied). But this is surely no exhaustive disjunction; one might say a thing is divisible, without at all

[1] I do not, of course, mean discernible by sense.

meaning either of these things. That he need not be asserting the *physical* possibility of a particular division is precisely what is intended by the qualification, " in thought " : and that he need not *merely* be halving some numerical interval assigned to it is indicated by the fact that a sufficiently bright person, wholly ignorant of any numbers or mathematical law, can in an obvious way sunder a thing " in thought ", i.e., can simply imagine it as separated. Indeed, it would be difficult to imagine two things less alike, and less liable to confusion, than the product of a mathematical law, on the one hand, and a temporal process, such as someone's running from A to B, on the other. Nor does the notion of smallness introduce any difficulty here ; just as one can thus break an apple, for instance, in thought (and ordinarily would do so, without bothering with numerical intervals, before breaking it in fact), so also one can think of any object or magnitude whatever as capable of diminution, or as separable into parts, without at all utilizing numbers. If, moreover, we can thus *think* of an object without actual contradiction, (however difficult it may be to the imaginations of some), what proof can be given that an object is not such, in fact ? What reason can be given for saying that a stone, for instance, *can* not consist of infinitely many parts ![1]

Now Mr. Black's proposal amounts to a denial of this. For him, a temporal process—e.g., running from A to B—consists of a finite number of acts, i.e., steps, which is of course true. But now we can ask, Is such an act itself a temporal process, or is it not ? If not, then its beginning and end are coincidental in time, i.e., the act is completed instantaneously, and hence coincidental in space as well. How a runner would arrive at his goal by such " steps " as these I leave to better minds to determine. If, on the other hand, each such act *is* a temporal process, its beginning and end are spatially and temporally distinct, and in no relevant respect does it differ from the whole *act* of running from A to B—which act was correctly described as divisible into smaller temporal processes. The relative size or duration of such an act is, again, quite irrelevant, so long as it is thought of, as it must be, as having any extent or duration at all.[2]

[1] Cf. Leibniz, *Monadology*, 65.

[2] Mr. Black's treatment of this paradox is essentially no different from that of William James. James wrote : " Either we must stomach logical contradiction . . . in these cases ; or we must admit that the limit is reached in these successive cases by finite and perceptible units of approach—drops, buds, steps, or whatever we please to term them, of change, coming wholly when they do come, or coming not at all . . . The infinite character we find in it is woven into it by our later conception indefinitely repeating the act of sub-dividing any given amount supposed. The facts do not resist the subsequent conceptual treatment ; but we need not believe that the treatment necessarily reproduces the operation by which they were originally brought into existence " (*Some Problems of Philosophy*, p. 185).

My own manner of removing the paradox raised by Zeno must be apparent from what I have said already, and thus requires little elaboration. It is not novel; as pointed out before, it was adumbrated by Aristotle. It consists simply of adhering to the notion of a discrete infinitude as a collection which has not both a first and a last member, and of not confusing difficulties of imagination with "self-contradictions".

Suppose one were told to enunciate the members of the series 1, ½, ¼ . . . etc. Now as Mr. Black perceives, this would seem to be a command to do the impossible; it would seem to take "forever" to do this, which is equivalent to saying it could not be *done* at all. This evident impossibility, however, arises solely from the physical limitations upon the rapidity of our thought and speech; we do require a certain minimum of time, and no less, to enunciate a number. Suppose, then, these limitations to be removed, such that, for instance, we could pronounce "one" in a minute, "one-half" in a half-minute, "one-quarter" in a quarter-minute, and each succeeding number in half the time of the former, not decreasing the rate of acceleration in our speech; would not every impediment have been removed? For it could then be asked, (1) How long will it take to enunciate the series? The answer to which would quite obviously be, not "Forever!", but "Two minutes"; and (2) How many numbers will have been enunciated? to which the answer would as clearly be, "Infinitely many".[1] Whatever difficulty this may present to the imagination, there is, I submit, no self-contradition, except on the gratuitous assumption that a discrete infinite series ought to have both a first and last member, which by definition it cannot. And the difficulty to the imagination tends also to diminish when it is borne in mind that, on our hypothesis, an ample number of times are provided for the enunciation of any and all members of the series, without exceeding the two minutes available.

Now it might still be asserted that a process having no last discernible phase has, by virtue of that fact alone, no "end", and *must* accordingly take "forever". But this would be to exploit an ambiguity in the notion of an "end". That a series has no last discernible member, and hence no "end" in that sense, hardly entails that it does not terminate, and must therefore last forever—the distinction between infinitude of division,

[1] This example is derived from R. M. Blake's paper, "The Paradox of Temporal Process", *Journal of Philosophy*. Vol. 23, No. 24, 1926, p. 651. A similar one is used by Bertrand Russell, "The Limits of Empiricism", *Proceedings, Aristotelian Society*, n.s. Vol. 36, 1935–36, p. 144. Russell suggests that it is "medically" impossible, rather than logically impossible, to enumerate an infinite series, (p. 143).

and that of extent, must again not be lost sight of. Indeed, *no* process conceived as divisible into intervals of the series 1 minute, ½ minute, ¼ minute . . . etc., *could* last forever, though it assuredly *is* " endless " in the sense of having no last discernible member.[1]

These remarks will, I believe, remove Mr. Black's specific difficulties, but it might finally, still be objected that Zeno's problem remains, inasmuch as no explanation has been offered of how any process " could ever get started ". But here, it must be clear, the problem is the same, except that the division is carried backward instead of forward (in thought, to be sure), so as to have, by this means, no *first* discernible phase. It is certainly true that if a process is conceived as divisible into intervals of the series . . . ¼ minute, ½ minute, 1 minute, then it surely has no " beginning ", if by this is meant no first *part;* but this hardly entails that it does not begin, and must therefore have been going on through all past ages, if at all. Indeed, no process divisible into *this* temporal series *can* be reckoned as having been going on through all past time, and must accordingly, be thought of as having originated—just two minutes before its termination. There are, I believe, no extra difficulties in this manner of conceiving the problem, which have not already arisen in the previous formulation, and those I hope to have removed.

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COMMENTS ON STORER'S DEFINITION OF 'SOLUBLE'

By GUSTAV BERGMANN

"CAN one, *within the formalism of Principia Mathematica,* construct the *contextual definition* of a predicate, '*S*', in terms of two other predicate constants, '*W*' and '*D*', so that if '*W*' and '*D*' are interpreted as 'put into water' and 'dissolves' respectively, '*S*' becomes an *idiomatically accurate* transcription of the English word 'soluble (in water)'?" Storer[2]

[1] See R. M. Blake, *op. cit.*, p. 652.

[2] T. Storer, "On defining 'soluble'", *Analysis*, Vol. II, p. 134–137. Storer's definiens for '*S*(x)' reads

$$\overrightarrow{W}D(x) \vee (\exists f)[f(x).(\exists y)\langle f(y).\overrightarrow{W}D(y)\rangle.\sim(\exists y)\langle(f(y).\overrightarrow{\overrightarrow{W}D}(y)\rangle].$$

Except for a reference to time which is not essential to the argument, '$\overrightarrow{W}D$' and '$\overrightarrow{\overrightarrow{W}D}$' stand for 'is put into water and dissolves' and 'is put into water and does not dissolve' respectively.

proposes a definition which he believes does the trick and is thus led to answer this familiar question affirmatively. If he is right, then the definition he proposes amounts to an analysis, in a familiar sense of 'analysis', of the so-called dispositional terms that combines, very remarkably, three characteristics. (*a*) Unlike Carnap's in *Testability and Meaning*, Storer's '*S*' is eliminable. (*b*) Unlike mine in the paper from which he quotes, it is an idiomatically accurate transcription. (*c*) His definition is, as he puts it, truth-functional. Or, as I should rather say, the primitive signs of the formalism he uses are, except for parentheses, either connectives, or individual and predicate variables, or operators and constants corresponding to such variables. Logical atomism, to use Russell's term, is the thesis that this syntactical apparatus suffices for the analysis of all philosophical puzzles by means of a formalism. The limitation is significant. A logical atomist could not, for instance, admit an unanalyzable term of causation. For 'causes' in the pattern '*p* causes *q*' is neither a connective nor, since it combines two sentences into a sentence, a (relational) predicate.

The novel feature of Storer's definition is its impredicativity, the use he makes of the '$(\exists f)$'. Because of this clever innovation his transcription is *more* accurate than any other of which I know among those that fulfill the conditions mentioned under (*a*) and (*c*). But it is *not completely* accurate, as may be seen from the case Storer himself mentions of a character none of whose instances ever have been or ever will be put into water. Let 'f_1' be the name of this character and 'g_1' that of another whose instances are, by assumption, soluble. One sees easily that '$f_1 \vee g_1$' verifies the scope of the existential operator in Storer's definiens. It follows that any kind none of whose instances ever has been or ever will be tested is, upon his definition, soluble, which is not in agreement with idiomatic usage. The same holds, in fact, for any particular, designated by '*a*', that never has been and never will be tested. To show this one merely has to take '$ca \vee g_1$' instead of '$f_1 \vee g_1$'. Storer seems to be aware of this, for he suggests that the range of the quantified variable in his definiens "should be restricted to 'natural' predicates; *i.e.*, undefined predicates or predicates whose definitions do not involve explicit reference to an individual". This, however, raises new difficulties.

For the condition mentioned in (*c*) to be fulfilled, any restriction imposed on the range of the predicate variable would itself have to be expressed within the formalism. In other words, the second disjunction term in the proposed definiens would have

to be replaced by '$(\exists f)$ (. . . and ——)', with ' . . . ' standing for the formalization of the restriction and ' —— ' for what is now the whole scope of the operator. But the restriction Storer suggests and which, I think, is of the sort one would have to impose, is in the nature of things syntactical or perhaps, as some would have it, semantical. In either case, the formalism would have to contain among its particulars some that could be interpreted as the names of predicates and among its predicates, for instance, one that could be interpreted as ' undefined predicate '. That a formalism so rich in expressive possibilities can be constructed within the limits mentioned in (*c*), that is, with the syntactical apparatus of logical atomism, is much too controversial a matter to be taken for granted implicitly. I, for one, happen to be of the opinion that the difficulty is not insuperable; but I shall, of course, not argue this very complicated issue in this note. I turn, instead, to a further difficulty that would remain even if the one just mentioned proved less serious than some believe it to be.

The occurrence of the word ' natural ' in Storer's informal statement of the restriction I have quoted is not accidental. All problems of formalization apart, the idea is to admit into the range of the operator only predicates that would " naturally " occur in " laws of nature ". This raises the question whether one can *precisely* indicate a class of such predicates that is also sufficiently broad to make the formal definitions of dispositional terms completely accurate or even as accurate as Storer's, as we now see, semi-formal one seems to be. The point is related to another on which he himself touches when he expresses confidence that his device, the use of impredicative functions, " can readily be generalized to all cases of counterfactual conditionals ". One of the questions currently discussed under that heading may be stated as follows : " Can one, in a formalism not containing an undefined term of causation or an equivalent symbol, distinguish syntactically between two kinds of sentential expressions, those which, after the formalism has been interpreted, we would, if they were true, call *accidental generalities*, and those which, if they were true, we would call *laws of nature*? " As far as I can see, nothing in Storer's proposal is either positively or negatively relevant to this question ; and the confidence he expresses is in this respect, it seems to me, entirely unsupported. Nor is this all. The answer to the question I just stated is, rather obviously to me, negative.[1] But if this is correct, then there is good reason to doubt that one could give a syntac-

[1] See also F. L. Will, " The Contrary-to-fact Conditional ", *Mind*, 56, 1947, 236–259.

tical (or, for that matter, semantical) characterization of the predicates occurring in laws of nature which is precise, sufficiently broad, and, at the same time, excludes such tricky ones as the two I used before. (It is this latter idea that I expressed by the phrase 'occur naturally'). For it is, at least, an essential part of the idea of laws of nature, in contradistinction to so-called accidental generalities, that they assert connections between two or more such predicates. I conclude, then, that while Storer's proposal is interesting, it would not be quite safe to rest the case for logical atomism, as far as counterfactuals are concerned, on it alone. So I still prefer to argue, as Storer correctly understands me to argue, that the purposes of philosophical analysis by means of a formalism require neither an idiomatically accurate transcription of the dispositional terms nor the possibility of distinguishing, in the manner indicated, between accidental generalities on the one hand and laws of nature on the other. There is, after all, such a thing as the fallacy of misplaced exactness; and these, I believe, are two instances of it.

I should like to make one more point concerning the dispositional predicates and counterfactuals in general, which is not related to Storer's paper. Philosophical analysts are well advised to avoid in the body of their arguments the use of such traditional labels as, say, "realism" and "phenomalism". But they would carry their abstention to an unreasonable extreme if they did not use what they have learned in this manner to clarify, not only the various possible meanings of the label words themselves, but also the patterns of thought and argument that are, historically as well as psychologically, associated with them. The current debate on counterfactuals is, I believe, a proper occasion for such a comment.

One may reject Carnap's proposal to introduce (not define !) ineliminable terms by what he calls reduction pairs and yet admit that such a pair, say, in the illustration at hand, the two sentences

$$(1)\ W \supset (D \supset S), \qquad (2)\ W \supset (\sim D \supset \sim S),$$

reflects accurately the truth relations among the three idiomatic expressions involved. To show this more clearly, I use the sentential tautologies

$$'(p \supset q) \equiv (\sim q \supset \sim p)' \quad \text{and} \quad [p \supset (q \supset r)] \equiv [q \supset (p \supset r)]'$$

to transform (1) and (2) into

$$\sim S \supset (W \supset \sim D), \qquad S \supset (W \supset D).$$

In each of these two sentences the antecedent, which is now either the dispositional term itself or its negation, says "more"

than the consequent in the familiar sense that a conditional is true when its antecedent is false and its consequent true. This " more " or, as some would put it, this excess meaning of the dispositional terms is, I suggest, the reason why the impossibility of defining them so that the conditions (*a*), (*b*), (*c*) are all satisfied is now sometimes used as a stick to beat the " phenomenalists " with what I take is a " realistic " intent. If " phenomenalism " means what it usually means, then these blows miss their intended victims. A phenomenalist, I understand, may accept as undefined or unanalyzable only predicates that refer to characters exemplified in percepts (or other " states of mind"); he undertakes to define or otherwise " introduce " all others, including those exemplified by physical objects. The idea is, apparently, that '*W*' and '*D*' stand paradigmatically for the former kind of predicate ; that '*S*' stands for the latter ; and that the phenomenalist cannot, *because of his phenomenalism*, give a satisfactory account of the dispositional terms. Clearly this is not so. Storer merely follows established practice when he interprets all three terms as referring to characters of physical objects. The point is this. The issue of the counterfactuals, if it is an issue, has to be faced also when one starts from a " realistic " class of undefined predicates as long as one is not ready to include into this class many more expressions than seems reasonable or, for that matter, many more than most realists would be willing to include.[1] It is clear, then, that what underlies the counterfactuals debate is not, as some seem to believe, the realism-phenomenalism controversy but, rather, the issue of logical atomism and nothing but this issue.

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[1] See also P. Marhenke in *Philosophical Analysis* (M. Black ed., Cornell University Press, 1950), p. 302 ; N. Goodman, *The Structure of Appearance* (Harvard University Press, 1951), p. 103.

ANNOUNCEMENT—ANALYSIS 'PROBLEMS'

Since most readers of ANALYSIS are in fairly general agreement about the aims and methods of philosophy, it has been decided to try the experiment of setting from time to time a limited and precise problem to be 'solved' in not more than 600 words. By this means, it is hoped, some small, but quite definite results may be obtained. Nor should these be despised as trivial. Though small in compass, they may be large in implication. At least it should be useful for contributors to exercise their wits publicly in the same areas. The Editor of ANALYSIS will retain general supervision of the scheme, but will be at liberty to choose, if desired, a special setter and judge of Problems and successful entries will be published in later numbers of ANALYSIS.

The first judge will be the originator of the scheme, Mr. J. L. Austin, of Oxford, who sets the following problem:

"What sort of 'if' is the 'if' in 'I can, *if* I choose'?"

Entries (not more than 600 words long), accompanied by a stamped, addressed envelope, should be sent to **The Editor of ANALYSIS by WEDNESDAY, April 30, 1952.** *No entries should be sent to Mr. Austin.* Contributors may submit entries either under their own names or a pseudonym. A report, with names of successful entrants, will probably be published in the June number of ANALYSIS.

THE EDITOR.